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AN ESSAY

ON THE

FUNCTIONS OF LIFE;

COMPRISING DEMONSTRATION OF

A SUPREME INCEPTIVE FUNCTION

IN THE

ORGANISM OF MAN;

NEW VIEWS ON THE PHYSIOLOGY OF THE TRUE CAPILLARY SYSTEM:

ON RESPIRATION—THE BLOOD—ANIMAL HEAT—MUSCULAR MOTION—INERVATION—
NUTRITION—SECRETION, ETC.

WITH

CORRELATIVE REFLECTIONS ON COMBUSTIVE AND OTHER CHEMICAL AFFINITIES;
AND ON THE NATURE OF LIGHT AND CALORIC.

ALSO

A NEW THEORY OF COMBUSTION;

AND

INTRODUCTORY OBSERVATIONS ON THE ÆTIOLOGY OF ORGANIC AND
INORGANIC OPERATIONS.

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SURGEON, LICENTIATE IN MEDICINE, ETC.

In partibus corporis nihil inane, nihil sine causâ, nihil supervacaneum est.—Cicero.

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PREFACE.

The following pamphlet consists of a transcript *verbatim et literatim* of a paper written by the Author in the year 1835, and intended, as may be perceived, to take its place among the ephemeral papers of the time. How far he may be justified in giving it publicity at this distance of time, and in its present shape, must be left to the reader to determine. For himself he has merely to remark, that although four years have since elapsed, and his own views and opinions have undergone no change as to the subject matter of the Essay, he is far from being unaware that in the present stage of the inquiry it is much more fitted for further prosecution and research than to be placed before the eye of the reader. Seeing, however, that he could of necessity only defer to an indefinite future that more rigorous course of examination, especially experimental, which the subject seems so much to require and deserve: he has rather in the spirit of the proverb "Qui non potest quod vult, velle oportet quod potest," resolved to put into print a few copies of the Essay as it is, flattering himself that from the high importance of the subject to which it relates, combined possibly with the originality of some of the views propounded, sufficient interest will be found attached to procure for it the reader's attention: perhaps, also, his devotion to further inquiry. And should it be found to have conduced to such an end, should it be found to have contributed in any way, or in the slightest degree, to the advancement of physiology, in this its most recondite branch, then will the writer's aim be answered, nor will he consider himself either unexcused or unrewarded for having ventured to submit it to the test of public opinion.

144, *Tooley Street, London,*
July 1, 1839.

AN ESSAY

ON

THE FUNCTIONS OF LIFE.

Notwithstanding the rapid and brilliant progress made by modern physiologists in the analysis, distinction and discrimination of the uses, if not in the classification of the vast assemblage of functions constituting the vital system of man, it must still be acknowledged, that as far as regards that branch of the science most of all interesting to the Natural Philosopher,—*i. e*, relating to the *modus operandi*,—the philosophy of those functions—little or nothing has yet been effected—that it remains, in fact, enshrouded in pristine darkness, unapproached, if not unapproachable by the illuminating rays of science; and the *rationale* in this its more extended sense of vital or physical process has rarely been deemed otherwise than as beyond the reach of the human mind. This is a question, however, which need not be examined here. It will be sufficient to concede on the one hand that a limit to human investigation must exist, bounding that which may be known from what must remain for ever unknown, and it will be easily granted on the other, that to this point of perfection our knowledge in physiology is very far from having yet arrived. Perhaps, indeed, if we briefly reflect on the present state of this branch of physics, and that of the collateral sciences—in conjunction with the efforts made by physiologists, we shall hardly fail to be convinced that even a vast field remains unexplored—teeming certainly with difficulties most formidable, but not necessarily indomitable, nor beyond the scope of scientific research. Nor does it appear that in proceeding from a knowledge of the effects and mechanism of the functions to the investigation of the influences by which they are determined and sustained, although placed beyond the pale of *direct* observation, our ignorance concerning them should be deemed irremediable. It may be that for their discovery and demonstration adequate means, and especially the requisite data are not within our present reach; but whatever those influences may be, their existence can but be admitted, nor exists there, I suppose, at least any mathematical impossibility opposed to their being brought more or less within the sphere of our knowledge. Under present circumstances, and so far deficient as we are in the aids of analogy, and of the lights and tests of collateral sciences, it is true that no great and direct progress can possibly be made. Nevertheless, even the smallest advancement in this important direction cannot be an object unworthy of attainment; even the development of apparently the most trivial fact may prove to be an achievement of no small relative value to the future progress of discovery; and this consideration might well supply a stimulus for unabated research. As to the œtiology of vital or organic operations, we are certainly as yet destitute of any analogical knowledge as a means of guidance and assistance in their investigation; even chemistry in its present state makes but a distant approach in this respect; it supplies us indeed with secondary explanations of several phenomena connected with vitality, and also with the knowledge we possess of the structure and elements of the organs concerned: but beyond this, it is obvious the principles of mere chemistry—apart from its philosophy do not apply. Still, however, chemistry must ever be the handmaid of physiology, and insufficiently advanced though she may be, constitutes an aid without which it would be in vain to attempt to institute any inquiry into the œtiology of vital operations.

The elements involved in the generation of animal phenomena are, we find, susceptible alike of chemical, as of vital stimulation—are affected, actively or passively—singly by either, or combinedly by both. It appears then sufficiently evident, that any knowledge as to the presiding causation of those phenomena can only be arrived at, if at all, through a system of investigation sufficiently comprehensive at least to embrace both species of influences. And omitting any anticipations as to the nature of these latter—the fact that they operate as they do on the self same elements of matter would necessarily conduct us at once to matter itself as the initiating point for inquiry. Now, what is to be understood by the word matter? In answer to this, it is necessary merely to observe, that it will be employed in this essay to designate alike both ponderables and imponderables; while the term substantive essence, will, for mere distinction's sake, be restricted to the ponderables alone. A question of more importance is—what is the nature of the part which the substantive essences play in the elaboration of physical phenomena? If we may premise that every effect necessarily involves a cause—every cause, a force, and that the word *force* represents one primitive fact only—we are first led to the general inference that physical changes and effects, however varied and complicated they may be, can only be attributed primarily to the impetus of attractions, (repulsions being, as I think, secondary) which obtain between the essences or bodies concerned. But how are these attractions developed? What is their source? Is the cause, *i. e.* the attractive faculty inherent in all ponderable essences? or does it exist in a portion of them only? The idea of its being confined to a part only, it is almost needless to observe is untenable both in theory and fact. Is any one of the substantive elements found as a constant in all combinations? Or is any known whose absence or presence suspends or restores the mutual properties of the other elements? Certainly not. Yet, however, it only follows that such might have been an opposite or antagonist to the attractor essences, but not that it could have been a ponderable,—if all ponderables are innately endowed with a one primitive faculty—attraction. The question then occurs—attraction for what? To this it can only be replied—for an *imponderable essence only*, and I shall presently prove that it must be so. As to the fact of the existence of such a principle as this, it is sufficient here to instance the electric principle as furnishing indisputable evidence thereof, whose presence is at least as co-extensive as ponderable matter itself, if not universal throughout all space, and moreover it might be safely added between which, and all ponderable matter hitherto discovered, the attractive affinity so implied, manifestly or discoverably, may be proved to exist. If it be granted in theory or fact, that the ponderable elements possess an inherent faculty of attraction for the one insubstantive principle, though in various degrees in the different essences, for so it must be if their properties differ, does it not therefore inevitably follow, that such must be their sole primitive faculty?—or shall we attempt to argue against the singleness of the nature of the causative faculty merely from diversity of the effects? I would ask is it in reality possible that any given essence can possess more than one specific faculty? and although a million may enjoy the same, is it possible that such function should be exercised except for one essence only of a contrary nature—different in fact from the given one itself, and therefore by consequence from all others. Considering the question on rational principles, it does in truth appear to me that we are bound to pronounce that it is utterly impossible for any one natural essence to possess or exercise primitively more than one special faculty or quality, and whether such be one of active or passive affinity, that it must be so unchangeably and exclusively. And further, therefore, that while it is sufficiently easy to conceive how any number of essences differing only in degree of the one primitive endowment, should each and all attract one essence or antagonistic principle, distinct in nature from that of the attractors, it is quite impossible to admit that such attractors, primitively or *per se* can possess any affinity at all for each other, and still less so that they possess both faculties, which would be to ascribe to one essence or power at least two distinct and opposite qualities, *i. e.* attraction for an essence whose nature it is to *attract* - attraction for an essence whose nature it is to be *attracted*. True it is, that the substantive essences *de facto*, when combined with the imponderable antagonist, do one and all attract, as they likewise, under certain circumstances, repel each other; but does it therefore follow, or can it be demonstrated, that such results should be attributed immediately and exclusively to the substantive elements themselves, and may not be a consequence due to their alliance with the secondary principle? Certainly not. On this point then, it is unnecessary further to remark, than that it leaves untouched the present argument, and we are therefore entitled to the necessary conclusion—that the substantive essences can have but one faculty, that faculty being one of attractive affinity; that all possessed of such faculty must necessarily be of *one* nature—the antagonist of *another;* that *both* are essential to the phenomenon of attraction, and that there can consequently be *no* innate attraction between *similar* essences.

To sum up—it would result from these brief considerations, that the whole of the natural essences are separable into two grand groups or divisions. 1. Attractors. 2. Common attractee. And it may be added, that as the attraction of the former, for the unity of nature of the latter, is a test of their own singleness of nature, so likewise is it a proof that they can have *no other* affinity than for that *one* common, and therefore universal antagonist. Finally, this reasoning is equally applicable to the nature of the common attractee itself, and appears fully to be confirmed by all that is known of the habit and character of that principle. Such, then, are briefly the views proposed to be adopted, touching fundamentally the œtiology of all physical phenomena and effects, not of course excepting even gravitation and aggregation—a point, however, and especially as regards the former of these, of but small importance to the main subject of this essay. If we admit the inferences already drawn—of course it follows that the agency of a secondary principle is essential to the elaboration, not only of physical phenomena in the ordinary acceptation of that term, but absolutely of all effects, all appreciable changes and combinations of matter whatsoever; and whether that agency be exercised by living or dead matter—as the function of a vital organ, or as the property of an inorganic mass, matters not at all. The changes produced must, in all cases alike, still depend on the presence of the one secondary operative principle of causation, as an ever essential link in the chain of cause and effect, for as it has been already shewn, the same argument which affirms the existence of one common antagonist, excludes entirely the possibility of the existence of any other; and thus so far then is amalgamated at once the œtiology of all actions, processes, operations—organic—inorganic—vital or physical — into one common question; whose general constants, could they be discovered, form the essential basis of one universal governing system or code of laws, differing from each other in respect only of the infinite unknown complications of other elements. Now if the attainment of any knowledge as to the laws presiding over natural phenomena be not impossible in any case, it cannot, I think, *à priori*, appear less practicable, nor certainly should the inquiry be less interesting, as regards operations termed vital or organic, than those which are purely chemical or inorganic, and it is not unreasonable to suppose, moreover, that the results may or might prove equally of general as of physiological import. I wish not to under-rate the difficulties which beset this subject. They cannot, perhaps, be over estimated—nor yet to deal in frivolous assertion; but I would certainly say that it appears to me, to say the least, probable, that by a well directed and searching system of inquiry and experiment—even in the present state of our resources, important accessions might be made to our knowledge, as to the œtiology of several of these and other natural operations with which we are as yet but slightly, or not at all acquainted—and perhaps, too, a path be opened, which, with the march of universal knowledge, might lead, at no distant period, to the progressive solutions of many of the most puzzling problems in natural philosophy. Such is my humble opinion, entertained, doubtless, in common by many others. To do honour to such a task—even with all appliances to boot—can only be the work of a master genius, whom perhaps we have yet to look for—a Newton, a Hervey, or a Hunter: still there is no reason why the limited effort of the more humble votary of science might not prove serviceable, in some degree, towards the same important end, and even failing, he would stand excused in the attempt.

Now, as regards the functions of the living tissues, I would first observe, that although they constitute an essential element in the conditions of development of all vital phenomena, they do not form, solely and uniformly, the whole causative power. They are sole generators only of the strictly animal operations: while of the processes of the organic economy, they evidently make but a part cause, and the other integrant is supplied from the nutrimental elements. Nor, although both species of influences are essential to the completion of organic causation, does any one definite causative part or species of power pertain to either exclusively; each species of relative power, as shewn by the results, being common to both systems of matter: so that, between the living and the dead series of influences or attributes, no line of division founded on *causative distinctions* can therefore exist, and we are, in fact, only enabled to contemplate them in the main, as a mixed, an heterogeneous assemblage of simple relative powers, probably of two kinds, and the attributes of two systems of agents: but not hence distinguishable in their causative capacities. However, as regards any œtiological investigation in respect of the vital organism and its effects, a somewhat general distinction touching the presiding influences may perhaps be observed and conveniently adopted, corresponding with the living and dead agencies. Thus, the vital powers or properties, whether in their nature active or passive, profluent or assumptive, &c., may, nevertheless be viewed as an *ensemble* of means primarily for determining those changes in the alimental elements which operate the effects; and so, in like manner, the properties of the elements may be regarded as furnishing the proximate correlatives or secondary powers, qualified in this point of view alike, whether by action or passion, profluence or assumption, &c. Hence, then, the living and dead faculties or powers, in reality heterogeneous as to their mode of action, admit, as occasion may require in the progress of inquiry, of being ranged in relation to their ultimate effects into *remote and proximate causes.* Now, as it is incontestable that the sources whence spring all the operations ministering to vitality can be no other than the relative properties of the living tissues, on the one hand, and those of the inorganic elements, on the other, (and further than this it would be folly to go), a few plain questions here present themselves for consideration. Is there a possibility that any light may be elicited as to the nature, development, and *modus agendi* of these powers, viewed in connexion with the origin and elaboration of animal phenomena and effects? Is it possible that a careful study of the latter may be rendered the means of disclosing any etiological facts, either of partial or general application? Can there, in fact, be any reason why such an inquiry may not be instituted into the operations of matter as they are displayed in the organic processes—as by the aids of reason, analogy, &c.—to determine in some instances the nature of the primary causific impulsions? Can we discover aught as to the conditions of development of the attributes which distinguish the living and the dead series of agents? Further, whether or not in their nature and laws of development these ultimate influences may be diverse or identical; and if identical, whether the inference have not a common application, and imply not, as regards such essentials, the grand corollary of their universality? Again, as to the natural statistics of the operations of the organic apparatus. Is it practicable to determine whether in totality they may be accounted as a congeries or a series; and if the latter, what also should be their classification—their order of development—their relation of importance? Is there or not a correspondence of the two latter? Finally, exists there or not any connexion that may be traced between the circumstances of their development? These points embrace matter of deep interest and importance, and in so far as so slender and imperfect an attempt may serve in lieu of a broad and comprehensive scrutiny, cannot be omitted among the objects of this essay.

To proceed, then, and uninfluenced either by rule or opinion, or alleged facts, let us now cast at once the net of inquiry into the living system of man. For this purpose, let us here suppose the blood (in our own body, for example) circulating at any given time, to be in a normal condition, and charged with a due quantity of natural aliment. We are assured of the healthy performance of the vital functions,—we know nutrition to be going on, and that ultimately organization is effected;—we perceive animal heat evolved;—the absorption of oxygen gas, and the extrication of carbonic acid gas, &c. The animal spirits are at the natural standard, and we conclude, by induction, that the nervous system is discharging its functions *naturally.* Under these circumstances, the stream of life—in harmony with nature—flows equably, calmly, and pleasurably. Such, it must be allowed, are the apparent results arising from changes constantly occurring in the animal system, and they are clearly ascribable, either separately or combinedly, to the animal itself, to its nutriment, or to the medium in which it exists. Now the animal itself can only be pressed into the service of this inquiry so far as its *physical agency* can be traced in demonstrable connexion with either of these results. It is clear, as we can have no action without matter, as matter cannot undergo any change without involving action, neither can it be the source of a series of changes without, at the same time, being the object of a corresponding series of changes, it becomes clear that these constant changes must arise from a constant intermutation of particles of matter determined by the essential properties of such matter. The only matter that can by possibility be concerned in the processes subservient to animal organization, must consist of, or be derived from, the atmospheric air and the *materia ingesta*. These, by certain well adapted contrivances, are first brought into mutual contact: their *first* meeting takes place in the blood, and there the *first* changes and combinations which occur between them are found to be produced. What are those changes? What is the cause, the *extrinsic* cause of their development? This last is indeed an important question. We know that between the alimental elements and the oxygen gas, the former being a compound combustible, and the latter the supporter—by the presence of a specific temperature as *exciting cause*—a pyrochemical affinity might be produced, which should determine their *igneous* combination: but it is clear that this species of combination does not take place in the organic system. Nor can the cause, the required specific temperature there exist, even supposing the combustible should be alcohol or hydrogen gas. Now, if alcohol, &c. combine with oxygen *in* the organic system, and do not *out* of it, independently of specific temperature, then certainly the cause, the *extrinsic* cause, or, in other words, the physical agency capable of producing the results, has yet to be sought, within the animal organism itself.

The differences between those results which take place between a combustible and supporter under the influence of specific temperature and those from organic excitement, are alone conclusive as to the dissimilarity of the exciting causes: they also go further. Let us see how far analogy holds between the two processes, (the one I shall call pyrochemical, and the other neurochemical), availing ourselves, at the same time, of the opportunity of testing the prevailing theory of combustion, as advanced by Lavoisier, modified by Gren, Leonardi, Richter, and perfected (?) by Thomson—a question which is indeed inseparable from the discussion of this subject. When the pyrochemical union takes place, the apparent results are: the combination of the combustible (say carbon) with the supporter (oxygen), forming the product (carbonic acid gas) with (by the disengagement and reunion of light and caloric (?)) the extrication of fire. During the neurochemical union too, the combustible (carbon) combines with the supporter (oxygen), the product (carbonic acid gas) is likewise evolved, and a part at least of the caloric is also liberated: but here the analogy ceases. To say nothing now of the presumed deficiency of the caloric—what becomes of its congener, the light? what of the fire?—produced (in the former instance) by their disengagement and reunion? The doctrine of the phenomena of combustion is this:—

"The component parts of the oxygen gas of the supporter are two: viz. 1. a base; 2. caloric. The component parts of the combustible are likewise two: viz. 1. a base; 2. light. During combustion, the base of the oxygen gas combines with the base of the combustible, and forms the product, while at the same time the caloric of the oxygen gas combines with the light of the combustible, and the compound flies off in the form of fire." (!) Hence, then, combustion is a double decomposition, determined by the positive influence of a specific quantity of caloric. Observing what occurs in the neurochemical process, we however learn—1. The existence of a different exciting cause; 2. The total absence of fire or light, and a part of the caloric from the results. This deficiency of the normal quantity of the caloric seems indeed sufficiently apparent, as well as from the diminution of caloric by the other processes of the organism, as from the very nature of the exciting cause of this, and being, as it is, a point of no present moment, it is superfluous to anticipate further proofs. Certainly, however, as regards the hypothesis asserting the distinct materiality of light and heat, if it might be difficult to account for the absence of *all* the light from the results of this process, to account also by the same theory for the absence of any of the caloric, must be impossible; but of this anon.

And now let us proceed to investigate the *nature* of this exciting cause, this *physical agency*, which has been traced to exist in the vital organism. In what then consists this power? The inquiry need go no further here than to its influence during the continuance of vital action. Now, I have called the process or set of actions occurring between the oxygen gas of the atmosphere and the elements of the combustible nutriment—by *virtue of vital excitement*—neurochemical, and which I consider as the *first set* of the organic series of phenomena, the source of simple vitality, the results being—1. The combination of the oxygen of the air with the compound combustible of the blood; 2. The formation of certain products; 3. The preclusion from those products of fire, *i. e.* light and caloric; 4. The extrication of free caloric; 5. The absence of fire or light, and probably a part of the caloric from the *manifest* results. Is the exciting cause positive or negative, active or passive, or, to speak more intelligibly, profluent or assumptive in its nature? In answer to this, it might be said, that the simple fact of the mere absence either of fire or of light, or of a portion of caloric from the results, might constitute alone a sufficient proof that it is *negative or assumptive.* But, independently of this, when we also reflect that the organization of the entire animal fabric is a result which follows, must in reality be the consequence of that process, without which there could be *no* organization, it becomes perhaps frivolous to doubt. It must be conceded that the primitive, the antecedent exciting power, cannot be the profluence of matter; (whence the supply?) *i. e.* cannot be positive in its agency; *ergo*, the blood being the sole source of the materials, the pabulum of the whole vital organism, the remote or exciting cause of this process, the primitive operation on the vital fluid, the fundamental *inceptor* power of the organic phenomena, is one and all *a passive or assumptive agency*. Again: were it not so, it would indeed be difficult to conceive how it is that combustion should not take place between these same elements within the system as it does out of it, either from the operation of the disposing cause of the liberation of the igneous effluence (supposing it a simple), or from the re-union of the light and caloric of the materials (supposing it a compound). Now something in the one case must prevent that extrication or re-union which naturally takes place in the other, and I know of no possible agency capable of effecting this, except such as acting as an exciting cause to the combination of the remaining parts, would at the same time abstract and retain either the igneous effluence or its cause, on the one supposition; and on the other, either the whole of the light, or the whole of the caloric, or both, from their respective bases. I need hardly say that neither both, nor the whole of the caloric is abstracted and retained. On the other hand, there exists no evidence whatever of the dis-engagement of *any* light, and certainly not of fire; neither can we admit that the *whole* of the caloric is liberated. So, then, in the absence of fire, light, and a portion of caloric from the results, it appears to me, that beyond the former inference we are thus led to the inevitable conclusion, that the exciting cause of this process, the key, and the antecedent in series of all the other actions of the living organism, consists in the faculty of abstracting and retaining either the simple principle of pyrific effluence, or the light and a part of the caloric of the combustible elements existing in the alimented blood. Having established these premises, it will be recollected that they involve the following positions:—that the oxygen gas of the atmosphere, the combustible nutriment contained in the blood, and the inceptive faculty of abstracting and retaining the simple principle of pyrific emission, or light and caloric, are the primitive and sole agents or sources concerned in the elaboration of animal phenomena. The question now presents itself—if, by adopting the theory of the distinct essences of light and caloric, we could regard the absence of light from the neurochemical results as attributable to a faculty of attraction for the lucific principle, would not the hypothesis exclude the possibility of accounting, through the agency of the *same faculty*, for any deficiency in the normal quantity of the caloric proper to those results? Undoubtedly. If, then, *there be really a deficiency* in the normal quantity of the liberated caloric, it amounts to this: either that the vital inceptive power consists of *two* faculties capable of abstracting, in certain unequal proportions, the essences of light and heat; or that it consists of a *single function*; and if so, takes up one simple principle only—*that*, which however we may look upon as double, can and must in reality be but *single*. And once admitting the singleness of this remote inceptive faculty—then do the existence of the one ulterior corresponding (pyrolucific) principle and the subversion of the theory of the distinct essences of heat and light, as well as that the assumption of that same principle may be an adequate exciting cause to the non-igneous combination of combustible elements, clearly and incontestibly follow. If, on the other hand, there be not a deficiency of the full complement in the caloric given out, why then it as clearly follows that the assumption of the lucific principle may be the efficient exciting cause to the non-igneous combination of pyrific elements, that light and caloric are indeed distinct bodies, and that the fundamental inceptive power of the living organism consists in the faculty of abstracting the former! It would thence appear, then, that could it but be proved by *direct experiment*, whether or not the full complement of caloric is equally eliminated in the neurochemical as in the pyrochemical combination, we might thereby arrive at once at the solution of two of the most important problems in natural philosophy.

But this unfortunately is a matter which, however easy at first sight it may seem, and how often soever it may have been decided to self-satisfaction, is nevertheless, I fear—nay, I am certain, quite unattainable in the present state of science. I do not of course mean to say, but that it may be easy enough to decide as to the amount of caloric given out from the whole of the vital organism combinedly; perhaps, indeed, this may not be otherwise than tantamount to the normal complement of pyrific development, such, at least, would seem to be the tenour of the results of recorded experiments. Yet if it be so, what would this prove? why, that although we cannot distinguish the results of the process in question from those of the other systemic processes, still if we set but *any* down to the account of the latter—and is it not clear that we must do so?—then of course so much *minus* of the pyrific complement must be the caloric of the former, and such experimental evidence, instead therefore of negativing, would in reality confirm the fact, and independent of all proofs, past or yet to be advanced, sufficiently justify, *per se*, the abandonment of the theory of the distinct essences of light and heat, as untenable and fallacious, presenting at the same time distinct proof of the simple assumptive nature of the primary causific impulsion, and (so far, therefore, the true character) of the fundamental inceptive power of the vital organism. It may be here remarked, that this power can now be viewed only as a faculty of attraction for one simple principle, of which light and heat are mere special impulsions, or actional moods of development, and which, in so far as it may have to come under notice simply in connexion with *those* results, it will be sufficient to designate, "pyrolucific," a term, moreover, which in the application of these views to the subject in hand, by attaching the meaning of light as an essence, *sui generis* (merging, for the sake of the hypothesis, the question of the loss of caloric—fatal to it) in the one case—and that of the *cause* of light as a property, in the other—may be rendered available to the purposes of both hypotheses, as far as the issue of the former, and without affecting the ulterior application of the latter. Before, however, proceeding to broach this part of the subject, it seems meet that having already given to the faculty a name, some attempt should also here be made to assign to it "a local habitation,"—to indicate, in fact, by what part of the organism such a function is or may be exercised, and likewise to inquire incidentally, whether it be an exciting cause of *other* organic actions or not?—or if it be one of a series, that the general nature of the connexion between organic causes and effects may be so far determined as to distinguish what may and what may not be the effects of particular causes, and thus to obviate a source of confusion when coming to speak of the primary impulsions and the resulting phenomena. In order to this it becomes necessary to enter in some measure into the broad generalities of the matter, and at the same time, it will be convenient to observe the conventional causific distinction formerly pointed out as based on the *living* and *dead* influences—the former being considered as remote or exciting, the latter as proximate or immediate causes. Let us commence the inquiry with the fact, that the phenomena elaborated by the vital organism are both *numerous and various.* Now what might we deduce from these circumstances of number and variation? If the effects are numerous at any given time, so of necessity must be their proximate or immediate causes, as, in fact, we know them to be. If the effects are variable in their character, they cannot arise from the same given number of proximate without involving the existence of corresponding variable remote causes. Again, we can conceive the existence of no physical cause that is not the attribute of matter, and if the same matter (in relation to the same proximate cause) can or does possess but *one* special property or faculty, it would follow that such remote agency would accrue from a number of remote causes corresponding with the variety of the effects produced, could only be the endowment of some matter in the vital system, which matter must necessarily be both extended in substance and variable in structure—characterised, in a word, by conditions found to obtain only in the nervous system. What part of the nervous organism is properly the seat of the function of organic inception? This is a question which can only be set at rest by experiment; but were I permitted to conjecture, I should certainly say, the "ganglionic system." Where is the function exercised? This, which is perhaps the more important question of the two, fortunately admits of a plain and easy answer.—The function is exercised, and the effects are produced, precisely at the junction of the arteries and veins on the internal superfices of the tubes composing the true capillary system.

Are the results of the organic movements to be considered as a congeries of effects arising from a synchronous operation of their causes on their first meeting, and inchoation of action in the system, independent of further changes in the proximate or consequent qualification of the remote causes? To assent to this would be to affirm, that from a given quantity of aliment, from the same series or dose of elements, the operation of the primary neurochemical process, the organization of muscle, bone, hair—in fact, all the actions of nutrition, with the respiratory and circulatory motions—digestion, absorption, the secretion of urine—all may be supplied and actuated at one and the same instant of time. But is it so? Certainly not. Several of the actions, as those of digestion and absorption, are generated previously to the entrance of the nutriment into the circulation, and having been received there, it is incontestible that the purposes of the neurochemical inceptor process are served prior, at least, to those of organic secernment; some of these, too, probably long before the others; and if the order of development should coincide with the relation of importance, we might say, that certain actions persisting *after death*, as the growth of the hair, nails, &c., constitute also the last links in the chain of animal organization. These facts, however, would seem to me sufficiently to warrant the inference, that although the actions of the organic economy are of unceasing occurrence, those incident to any given instant of time cannot arise from the elements of one dose or series of proximate causes as pre-associated in the nutriment; but probably from different ones, and according to a specific law of succession, the relation of natural action possibly being developed between each exciting cause or set of causes, and its corresponding proximate under special conditions only, consequent and accruing from the evolution of the antecedent phenomena, so that, in fact, it would thus appear that the exciting and proximate causes of a series of organic actions may be qualified on the one hand, and derived on the other from as many doses, as many definite sets of the component elements of a series of proximate causes, and such being also in as various states of association as there are sets of those actions composing the vital series.

Now, in this case the operations of the organism might, I suppose, be regarded as a concatenated series of sets of effects, having in relation to the development of their causes a beginning and an end, *i. e.* certain changes are effected in the proximate elements, by one set of actions, by which they qualify and are rendered fitted for a second set, and so to the last, presenting thus a differential successive series both of proximate and remote causes, producing in a certain order of sequence a corresponding series of sets of effects, determined by those changes in the proximate elements, and concomitant qualification of the exciting power, consequent on the elaboration of the phenomena antecedent in the series. But admitting this, still the conditions of the first set of actions cannot be so derived, and whence spring they? For if it may be alleged that we are enabled to trace out the adoption of means to *prepare* and *qualify* the proximate and exciting causes of the subsequent less important sets of operations throughout the system, we cannot surely suppose the omission of all for the first and greatest. In the latter case, the necessity should, indeed, be *a fortiori*, should be paramount; and as to the variety and extent of the required means, should they not be commensurate with the whole of the elements? The fact is, as analogous means would in this case be impossible, so would they be utterly useless if possible; we therefore find special and very complicated means provided, commensurate with the importance of the end to be attained, namely, on the one hand, the selecting, preparing, assimilating, and conveying from an heterogeneous mass of useful, useless, and injurious materials, those elements only fitted to form a series of proximate causes, to subserve the uses of the grand inceptive function, and of all the subsequent actions of the system. These means consist of the various actions, essential and accessory, connected with digestion, absorption, fæcal excremention, &c., and which would therefore appear to me to constitute a special additamentum or organic accessory system of actions, altogether distinct from those of the main series, the first of which alone is the spring of vitality, and the last, that of the last link in the chain of vital organization. For this purpose are these isolated actions found to exist, involving, as regards their proximate causes, those preliminary changes in the alimental elements incident to their qualification and transit for the initiatory process of the main organic series. While, with respect to their exciting causes, on the other hand, whatever their organism, being but the attributes of a special appendix of the organic system, they exist probably only by virtue of power derived from that system, and hence, therefore, are wholly subjected to the influence and control of the various states and conditions of all the other functions and processes of the system.

Such, then, are the general conclusions which have appeared to me to offer themselves from a cursory contemplation of the number, variety, and circumstances of the organic actions, and which, however vague and valueless in themselves, may not, I conceive, prove useless in relation to the *ensemble* of the present inquiry. They amount briefly to this:—That the exciting causes constitute a differential successive series of sets of causific impulsions, residing in the nervous system—all empowered by the agency of the first—the supreme inceptor; but each more or less modified by the actions preceding or following;—that the proximate causes derived from the ingesta, &c., form likewise a differential successive series, each set developed by the changes consequent on the operation of the preceding, except the first, which is qualified by the accessory system of processes—the special originator of all;—and that the effects being the conformable evolutions of these impulsions, participate of course in their common signality. I pass now from these remarks to pursue again the main thread of the inquiry on the first set of actions. I shall not dwell further on the relations of organic causes and effects. I here take up the one faculty which I have been already led to single out as the supreme inceptor of all vital phenomena—which I have deemed to constitute the first of the organic impulsions both as to time and importance, and which I would further venture to suggest, sustains a function essentially instrumental in the economy of all systems of vitality—whose influence forms a necessary, probably as a vital element, a solely constant condition of every living action. Now what is the nature—the true *rationale* of the changes which take place between this faculty and the alimental elements, including, of course, the oxygen gas of the atmosphere? For reasons already alleged, the first explanation which I should offer is merely this:—The inceptive faculty of assumption for the pyrolucific principle being brought to bear on the combustible elements of the blood, in contact with the inspired oxygen gas, effects their decomposition. The pyrolucific principle of the combustible is retained by the vivific nerve. The base consequently set at liberty combines with the oxygen, carbonic acid gas, &c. are formed, and the caloric is disengaged, which subsequently diffused through the fluids and solids of the body, constitutes animal heat. This then is the first neurochemical process, the first set of the series of organic actions;— the generator of simple vitality, and however viewed, and whether regarded as one of a series or congeries, must, in reality, be still the same. That this process should confer an influence which makes an essential condition of vital action, on the tissues comprising the whole system, and in what way, it is true has yet to be shewn. But as I shall have more to say in connexion with this anon, I would here merely offer the following conjecture:—Suppose that the faculty of supreme inception should be the attribute *ultimately* of *one* division (corresponding with the sentient) of a *double functioned* nerve—then what should be the province of the other division? Might it not be the office of *that* (the analogue of the motor division) to confer (whether directly or not) an essential positive qualification (be it irritability or not) with which, as a common element of action, the whole of the motive tissues may be endowed. In this case, the first set of actions, essential to simple vitality, being strictly those of the blood and the supreme inceptive function, the second, it might perhaps be said, would be those of the blood, and (indirectly) the muscles of respiration; the third (if not third and fourth) those of the blood and the right heart; the fourth (if not fifth and sixth) those of the blood and left heart; so that it should appear that the first neurochemical process, although as far as vital causes are concerned entirely dependent on and perfected by the first, extends thus beyond the third set of actions, before its completion takes place by the combination of the devivified carbon, &c. with the oxygen of the atmosphere in the pyrenchyma of the lungs. Be this, however, as it may, it is sufficient here to have canvassed our ground as far as the first set of actions, as the source of simple vitality, and to the development of the consequent neurochemical results, as of vital impulsion residing in the living tissue, the exciting cause, and of chemical impulsion, in the air and ingesta—the proximate causes.

Before quitting this part of the subject, perhaps a few moments might not be inappropriately devoted to the consideration of the influences by which are determined certain variations observable in the rate of progression or evolution of the fundamental phenomena of vitality. I do not here allude to variations in vitality, either arising from natural variation in the prime impulsion, as from age, disease, plethora, exhaustion, mental influences, &c., nor those owing to deviation from the normal state of the inspired air, as from factious gases, although well worthy to be noticed: but those only arising from the only remaining cause—namely, variation in the character of the aliment. That differences do occur in the intensity of the process of vitality according to the description depending in fact on the *pyrochemical character* of the aliment, must, I should think, appear too obvious to be doubted; thus, for example, the stomach may be filled with bread and water, or with bread and wine, but certainly with a quite different degree of vital progression in the one case from that which occurs in the other. Now, what is the cause of this difference of results? It is this—the only one, and one which is at the same time fully adequate to the effect—a *corresponding difference* in the pyrochemical, or, as might now be said, (*for they are equal*), the neurochemical affinity of the absorbed aliment, by necessarily inducing the correlative difference in the functional action of the vivific nerve, whence must be determined the ratio of evolution of the resulting phenomena. It is to be recollected that the supreme inceptor function of the vital organism is essentially a relative one; that its action is dependant on a condition of the nutrimental mass; that an immutable relation must therefore exist between the characters of these, and thus a given difference in the latter must necessarily be transmitted to the former, and consequently to the ultimate results. The higher, therefore, or the lower the pyrochemical affinity (*cæteris paribus*) as an index of the neurochemical affinity of the alimental elements may be, so will be the rate of evolution of the consequent neurochemical phenomena, and thus, all nutritive substances being combustible, the food of life, as we find the process of vitality varying according as the blood may be charged with this or that aliment, the reason for such difference may be seen in the pyrochemical character of the latter. To this I would merely add, that as the natural standard is of course to be found in our natural food, such may be deemed natural excitents. Alcoholic compounds are perhaps the lowest grade of preternatural excitents. The various æthers, as their pyrochemical affinity would indicate, are probably a superior class. While the pyrochemical character of phosphorus would seem to point out this substance as the highest of all the vital excitents yet known.

I come now to the consideration of the grand problem that has so long puzzled and divided philosophers. What is light—Caloric—Fire?

First as regards light. It will be necessary here only to speak of it as it exists in connexion with caloric in the constitution of fire. It will be as well therefore to premise that the term pyrific light be so understood—as referring merely to that which is evolved in combination with a corresponding amount of caloric (the pyrific or supra calorific), the joint emission of which is in fact its essential co-ordinate—in contradistinction from light, which may be otherwise and variously produced, and either unassociated with a fixed quantity, or free from any caloric. According to the philosophy of the present day, light is, I believe, generally considered to be a material principle, *sui generis*, distinct from that of caloric, and by the prevalent theory of combustion, it is the combined evolution of both these which constitutes fire. Light, it is said, is a component of every combustible—caloric, a constituent of all supporters, and by the double affinity and action of these constituting combustion are produced fire, and the products of combustion; so that the *rationale* of combustion turns on the hypothesis affirming distinct essences of light and heat, without which, of course fire cannot be formed, combustible and supporter cannot exist, and free from which all products must be. It is, perhaps, hardly necessary to observe that the mere fact of an hypothesis affording an explanation, even of the whole of the phenomena to which it relates, although furnishing a negative assumption in its favour, can be no proof whatever of its truth. The oxymuriatic theory, as is well known, applies equally to every fact with the chloridic; yet it is *not* the true theory, and a single exception might have superseded the laborious investigations of a Davy. Whether such an exception, in respect to the doctrine in question may, or may not be discovered, this at least is certain, that the fact of its accounting as it does for the phenomena connected with combustion does *not* amount to a conclusive proof of its correctness. Where, then, are the proofs of its correctness? There are none, and the physiological disproof already advanced as fatal to it, remains in fact, unshaken. But are we indeed certain that no facts are to be discovered, even among the phenomena of combustion, incompatible also with such an hypothesis? I think not. Suppose, for example, we could exhibit a "product" in the act of supporting combustion, or the direct combination of a "supporter" in the formation of a "product," &c., without the intervention of combustion. Either instance would suffice; and where then would be the ingenious theory under consideration, and what stronger argument for seeking the true explanation deeper still in the mysteries of nature? It appears from the following passage, which I find quoted from Ure's Chem. Dic., that "the most beautiful analytical experiment with carbonic acid is *the combustion of potassium* in it, the formation of potassa and the deposition of charcoal."

Now what does this mean, if it be not an instance in which a "product" supplies the part of a "supporter?" Let then this simple experiment be appealed to, and if such be in reality the nature of its testimony, it is enough, and away, at once, with the specious bubble! Sir H. Davy long ago announced the fact that combustible bodies may be made to combine rapidly with oxygen, at temperatures *below* what are necessary for their visible inflammation, or, in other words, without the intervention of combustion; not, indeed, yielding the usual "products" of combustion (a fact, by the by, only accountable by another theory), nevertheless, according to the postulate of the distinct materiality of light and heat, it must be clear that the oxidation effected could not possibly have been unattended by a corresponding expulsion of these bodies. Can it be said that *light* may be *invisible?* These facts alone, then, might suffice for the subversion of the hypothesis under discussion, the former being totally incompatible with the independent existence of heat, as the latter is with that of light; while the only inference legitimately to be drawn is, that they are both but *properties*, arising from actional moods, special impulsions of the *one* common ulterior operative principle, whose existence may be recognised in the electric imponderable, as the proximate agent or universal antagonist pervading all space. But these are not all the grounds for maintaining that light and caloric are in truth the mere properties of special imponderable impulsions, and that fire therefore can only be an electric emission embracing (whether by single or double evolution) both species of results. Does not electricity, in the decomposition of water, &c. &c., supply both? No one will deny that it does. Surely, then, it must be admitted either that light and caloric, or rather we should say their cause, constitute but *one* essence, or that fire and the electric principle also must each consist of two essences, in which case, too, the faculty of organic inception must consist in the assumption of light! Unless, therefore, the latter position be tenable, I can only again repeat the same inevitable conclusion, that the results are referrible to the operation of the same simple principle of causation, and are the immediate relative qualities of the imponderable ingredient essential to all physical power. I shall now quit the theory of the distinctive essences of light and heat; but before attempting at any explanation of the phenomena of combustion considered as electric impulsions, it will be requisite briefly to advert to the broader generalities of the question considered in relation to the laws common to all effects, and the basis of every special solution. I shall not attempt to enter here into that branch of the subject relating to the bulk, figure, and texture; the physical characteristics, which may be supposed to distinguish the substances, especially those deemed primitive, one from another. Whether such distinctions exist *ab origine*, are fixed and immutable, or whether they are the result, the stamp as it were of a co-existent combination of formative powers constituting in each case a radical constant, undecomposeable only by art, and therefore casting thence (whether singly or combinedly) *uniformly* with all concurrent influences those distinctive changes only which mark any given series of phenomena. It is true that as regards these distinctions, the various elementary attractors may either be considered in the light of primitive and unchangeable creations, or they may be set down as commutable elaborations, mere convertible consequences, dependant on corresponding ætiological influences, and in support of this, strong arguments might not be wanting; but that with the inscrutable problem here arising, it is useless farther to complicate the present inquiry which is in nowise compromised by the question. It is sufficient here to view the electric imponderable on the one hand as an universal antagonist, and on the other, the elemental attractors as a series of undecomposeable formations. I would then ask should it be possible to unveil in any degree, the nature of the connexion existing between such as attractor radicals, and common attractee? To deduce the laws governing their operations and assign the *rationale* for their countless variety of changes and moods of action as manifested in the phenomena of the physical universe? Obviously this must be a task in the main, altogether beyond our faculties. But although I admit this and that, little else than a falanx of hopeless difficulties presents at every point, still I am *not* prepared to contend that the investigation necessarily admits of *no* success, in which case indeed there could be hardly any proof, certainly no practical value conferred on the abstract hypothesis, however true in itself, to which it relates. In reference to the difficulties of the subject, it may here be remarked *en passant* that besides our inability to determine the existence of a large proportion of the intermediate facts which would constitute the requisite data; such, also, is the complexity of the relation of action existing between the essences concerned, that it would be in vain to attempt even to indicate its extent. How frequently are influences to be observed whose sources are completely unknown to us, and of what are known, each of the infinite variety of combinations into which the elementary particles are found to enter in the constitution of the substances with which we are surrounded—involves, at least, one new and distinct relation in the universal catalogue; added to which, we have the relation of masses, the source, probably, of the phenomena of gravitation, magnetism, &c. But, although fully impressed on the one hand, of the utter futility of seeking to unravel operations which may be the result of a falanx of imperceivable phenomena, and dependant on laws complicated, possibly, with every molecale of matter contained in the great crucible of nature. I still feel, on the other, that enough remains to incite, I hope to warrant, the true lover of science, however small his pretensions, to efforts of investigation and research; nor would I myself pretend to predict how far in the end the results may, or might not extend. Having arrived at the conclusion, that light and heat as developed during combustion, arise from peculiar impulsions of the electric imponderable, determined by laws springing from the fundamental relation existing between this principle and the essences concerned; and that all discernible phenomena are ultimately referrible to the same actuating power. The question comes now to be considered:—can any of those general laws be made out—which, as they belong to all matter, necessarily relate to every special process? Wholly dependant on this must be the theory which may be suggested in explanation of the phenomena under notice. Now, in accordance with the arguments already adduced, 'and without again touching upon the question as to the causes of their differences, I proceed here briefly to submit.—1. That the radical essences, being in reality all of one nature, although varying probably physically in figure, bulk, and texture—differ functionally one from another in no other respect than that of *attractive affinity*, or primordial capacity, for the electric imponderable or universal antagonist.—2. That all bodies, both simple and compound, have a quantity of this principle, varying with their conditions, allied to each.—3. That the exercise of this function, in conjunction, however, with the *modifying* influences of physical distinctions, must be regarded as the source of all their actions, properties, and combinations.—4. That the isolation of the substantive radicals from this principle, if it did not involve the utter dissolution of their physical distinctions, must render them, at least, perfectly inert towards each other.—5. That, from their combination with a *minimum* quantity of this principle, (probably by a specific disposition determined by the physical influences,) the attraction of aggregation is produced as a primary result.—6. That, from a farther combination ensuing, due to their general capacity, a state of equilibrium is produced, when each body may be considered to be in alliance with its natural quantity of the common antagonist.—7. Any departure, *plus* or *minus*, from this determinate quantity, naturally allied to all substances—confers, on them, (to say nothing here of other consequences,) what in the one case, I must call *passive* attraction, and in the other, *active* attraction, in reference to those previously in equilibrio with them.—8. The power of resistance of substances to such departure—*infra* or *supra*, from the natural standard must be inversely (*cæteris paribus*) as their equilibric capacities and the degree of aberation.—9. Different degrees of variation from the natural standard, confer (*cæteris paribus*, at least) corresponding degrees of general passive attraction on the one hand, and of general active attraction on the other.—10. Major and minor passives, and minor and major actives, will also be passive and active attractors to each other.—11. Equal degrees of variation (in similar essences or bodies,) above or below, like the natural standard, confer *inertia* with respect to each other.—12. While, in dissimilar essences or bodies, the degrees of departure necessary to the same result will, of course vary according to the difference of their primordial capacities &c. Thus much, would I venture to suggest, as common principles, in relation to the fundamental laws presiding over the development and force of attraction, distinct from the conditions of imponderable evolutions.

It is now for us to consider whether or not such an exposition be attainable, with respect to the latter, as in conjunction with the former, will help us, in any instance, to a solution of the questions raised, especially those connected with heat and light. It is clear that although attraction may exist independant of evolution, it coexists with electric, with galvanic, with calorific, with pyrific evolutions &c.; and for the elaboration of either of these phenomena, it is requisite merely in addition to a determinate degree of attractive force, there be likewise a special set of conditions. What and whence such conditions may be as regards combustion I shall presently attempt to inquire. But first, however, as regards the *force* of attraction, which for our present purpose I should here divide into two grades, the chemical and combustive. I take it that there exists a point of degradation on the measure of the force of attraction where (*cæteris paribus*) it fails for the purpose of combustive action, and becomes instrumental only for the production of chemical and other results. This point, I make the index of segregation for the above division, and suppose it to constitute the limits of what I shall call pyrific attraction, or that development of power essential to the evolution of fire. In illustration of this I may mention the instance of æther and oxygen, as between these substances we may have either pyrific evolution and a combustive product, or calorific evolution and a chemical product, according as either condition as to the force of attraction may be brought to co-exist at the time. Now as to the agents of combustion—chemical writers tell us that combustibles and supporters *are* positive and negative electrics, but it must be obvious that it is in a restricted sense only, if at all, that this can apply and that it would by no means hold as a general proposition. Thus for example—if the candle now before me, and the circumambient oxygen gas were positive and negative, or rather as I should say, passive and active attractors to each other:—Why stood the candle in need of being lit? and how comes it that the impulsion of a slight jet of breath should serve to extinguish it? While some few combustibles ignite at the common temperature—do not the vast majority require an intense degree of adventitious heat to develop that process? I shall content myself here with remarking that combustibles and supporters certainly do *not* necessarily stand in positive and negative relation previously to the development of combustion, that it is during the combustive process alone that such a condition exists—then, and then only, is it that such a relation of passive and active affinity is essential, and it forms too, as I take it, the very key of the process. I have already said in what the conditions of such a relation consist; namely, the difference *plus* or *minus* of the *existing* from the equilibric quantity of the electric imponderable allied to elements or bodies. But how such a state is brought about, as involving also the conditions of the development of combustion, in what manner it accrues from the process itself, and whether the whole be blendable into one consistent theory remains yet to be considered. As well as that each radical or body has its natural quantity of the imponderable antagonist, to which, under ordinary circumstances, it is invariably allied; it has also been suggested that the quantity must be variable in different essences, and that to these differences, determined by their formative distinctions, must be ascribed all differences of properties and actions. Were we to apply this to the specefic subject in hand, I should say then that where this difference, be it either natural or artificial, amounts to or exceeds a certain point, a force equal to or exceeding the minimum pyrific affinity would be set up, and pyrific action might be the consequence: the one body, positive or passive, acting as combustible; and the other, negative or active, as supporter; but that when the amount of this difference should fall short of this point, the degree of force generated with the consequent evolution would fall also so much short, and being therefore inadequate to combustive phenomena, chemical &c. results would only be produced. Such would appear to be the case among combustibles and their compounds, varied with products and supporters in various electrical states, as happens in the more active operations of chemistry, and probably as of the least force between products, especially those having the same supporter for their base. Now in attempting to analyse the inscrutable phenomena of combustion, the first fact I would notice is the necessity that exists for the application of a certain degree of temperature for the development of the process. This temperature or the amount of electric effect required is different both in cases of different combustibles acting with the same supporter, and in those of the same combustible acting with different supporters. Thus, phosphorous acting in air requires little more than the common temperature; sulphur requires a higher, hydrogen, carbon &c. still higher, while some of the metals are scarcely susceptible of ignition by the most intense degree of heat that can be applied. Hence then, we remark, that each combustible requires, in relation to the same supporter, a *specific quantity* of calorific or electric influence to throw it into the process of combustion. And why is it that one combustible requires a greater amount of electro-calorific influence than another to determine the process of ignition? My answer would be this. That, because as their attractive capacities for the imponderable antagonist vary, so also must the natural or equilibric quantities of that principle attached to each, and so consequently must the differences between these quantities and the common amount necessary (*cæteris paribus*) in reference to each supporter to constitute the conditions for the development of the *minimum* power of pyrific affinity, which involves the starting point of ignition. And as these differences too must represent the specific electro-calorific influences required; so likewise, it follows, that they must be in the inverse ratio of the natural or equilibric quantities. Of course, I do not mean to imply that these comprise the sole and only conditions of combustion. It does not follow that all essences or bodies, possessing the right conditions in this respect, should be capable (although essential to it,) of entering into ignition; nor therefore, are we to conclude, (for example,) that gold, platinum &c., may not in fact, be properly qualified in this respect to act the part of combustive supporters, to oxygen gas, and yet, combustive combination, be nevertheless, perfectly impossible between them. And this, for reasons, which although I pretend not here to discuss, cannot certainly be repudiated, even though they may be unfathomable.

The next fact is, that in all cases of combustion, the abstract electro-pyrific effluence equals, or exceeds, the specific influence; and so it is, that as the latter may be thence necessarily supplied, the process of ignition is, therefore, kept up as long as the materials endure. By the *abstract* effluence, is here meant the *whole* of the imponderable emission, eliminated from the combustive and supportive molecules, on their pyrific combination. It is in all cases equal, and must consist (1.) of the specific influence extrinsically derived, and which is imparted to the succeeding atoms; and (2.) of that which, remaining *free*, constitutes the pyrific evolution. The *free* pyrific evolution however alone is of course all that properly belongs to the ingredient elements of combustion. Now, let us here consider more particularly the source of this latter. It might perhaps be supposed that both combustible and supporter contribute to it in an equal degree: but such cannot be the fact; at least it appears so to me. Indeed, to say nothing of other reasons, the instance of the ignition of potassium in carbonic acid gas, seems to me to show this, and not only this, but that the influence of the supporter in that respect might even be set down as *nil*, and that the pyrific evolution might be regarded as an eduction solely from the equilibric of the combustible. This may be so. But what I have here to contend for is simply this:—that a constant ratio exists (*cæteris paribus*) between the equilibric of the combustible and the free pyrific effluence. These latter either may or may not be equal in amount. The equilibric of the supporter and that of the product either may or may not be in all or any cases equal. They may bear a constant ratio to each other, or be variously more or less than equal. And so also that of the combustible may either be given off wholly or not, or in a greater or less proportion—variably or invariably. These results *may* possibly vary with the physical conditions of the different cases, and so far qualify accordingly in particular instances; yet such variations can in no respect disturb or affect the abstract position which I have been led to adopt, and have now to maintain. Admitting, then, that a constant ratio may exist between the equilibric of the combustible and the free pyrific effluence, without uselessly attempting to show either that they are or are not *precisely* equal in amount, I may, as I am entitled to do, with this qualification, preserve—in the details of the hypothesis presently to be exhibited—the use of *one* numeral for the representation of *both*. But, first, the process of combustion may in a general way be thus explained. On the application of the electro-calorific influence required, the equilibrium pre-existing between the combustible and supporter is thereby at once destroyed. The combustible is rendered positive (or passive), to which the supporter, remaining still in *statu quo*, becomes consequently negative (or active). *Pyrific attraction* is thus set up between them, and determines their immediate combination; at the instant of which (or possibly as a sequence, and might be viewed as an expulsion from the product), the imponderable of the combustible, together with the specific which has been supplied to it, is disengaged. Of this, a part is absorbed as the specific influence of the next in series, and the rest extricated in the impulsive form of fire, constitutes the electro-pyrific evolution. The relation being thus changed, and the combustible diverted of its imponderable (at least down to the *minimum* quantity), that of the supporter still remaining, is then lost in the completion of the aggregate, or rather the equilibric, of the product. From the abstract being above the specific, the latter of course thus derives a constant and never-failing supply, and the process of ignition is consequently kept up as long as the materials endure.

Now, suppose 30 to represent the *minimum* pyrific power, or that difference of the imponderable quantities of two substances essential to the development of electro-pyrific attraction. Let the free electro-pyrific effluence correspond with the equilibric quantity of the combustible; let the quantity of the supporter (oxygen) be 10, and equal to that of the product; then the imponderable quantities of the equilibric—of the specific—of the abstract—and of the free electro-pyrific evolution in all cases of combustion having oxygen as supporter, might be thus exhibited in the following

TABLE OF PROPORTIONS.

Equilibric or Natural.	Specific.	Abstract.	Electro-Pyrific Effluence.	Equilibric or Natural.	Specific.	Abstract.	Electro-Pyrific Effluence.
40	0	40	40	20	20	40	20
39	1	40	39	19	21	40	19
38	2	40	38	18	22	40	18
37	3	40	37	17	23	40	17
36	4	40	36	16	24	40	16
35	5	40	35	15	25	40	15
34	6	40	34	14	26	40	14
33	7	40	33	13	27	40	13
32	8	40	32	12	28	40	12
31	9	40	31	11	29	40	11
30	10	40	30	10	30	40	10
29	11	40	29	9	31	40	9
28	12	40	28	8	32	40	8
27	13	40	27	7	33	40	7
26	14	40	26	6	34	40	6
25	15	40	25	5	35	40	5
24	16	40	24	4	36	40	4
23	17	40	23	3	37	40	3
22	18	40	22	2	38	40	2
21	19	40	21	1	39	40	1

Every substance whose equilibric quantity amounts to 40 or upwards, that of the supporter being 10, requires no specific influence to determine the process of ignition, or in other words, such substances are spontaneous pyrifics, being those in which the conditions of pyrific attraction and development already exist. Substances whose equilibric quantities range as in the preceding table, from 40 to 1, require respectively the specific influences in the inverse ratio, from 1 to 40, to fulfil such conditions, and consequently to determine combustion. These, therefore, when susceptible of combustive action, (that is, possessed of the requisite physical relations, and the application of the necessary specific influence being practicable,) comprise, of course, the non-spontaneous pyrifics. It is sufficient here to remark, that the combustive process will be accelerated in the ratio of any supra-specific influence applied, so that the higher the equilibric of the combustible, and the higher consequently the free pyrific effluence, so much the more rapid the process of ignition, and, of course, the greater the aggregate emission, and *vice versa.* The process of combustion is also retarded or arrested, probably mechanically, by any declension from the natural purity of the supporter from the admixture of non-supportive gases, &c.; hence, substances are found to burn more brilliantly in oxygen gas than in common air; while some, (as iron) capable of immediate ignition in the former, are but mediate combustibles in the latter, the rate of evolution being insufficient to raise the abstract aggregate effluence to the pitch required for the supply of the specific influence. But this too may happen when the supporter is pure, for as the equilibric of the combustible falls, so does the specific rise in the scale; and as the rate of evolution, and consequently the abstract aggregate effluence, decrease in the same ratio; a point of approximation is thus arrived at, where, the latter failing to impart the specific influence, combustion ceases, and, of course, in such cases is only to be maintained by a constant *extraneous* supply of specific influence, as by the admixture of superior combustibles, &c. Having said thus much as to the ætiology of combustion, one point, it is to be remarked, still remains unexplored—unnoticed. How do those distinct impulsions of, or dependant on, the electro-pyrific emission, productive of the *distinctive* effects of heat and light, arise? To this I can only answer that I know not, neither shall I here attempt the inquiry. It forms, in fact, but a secondary question, however difficult of solution, and as it may stand unconnected with the present essay, need not be here discussed. Now, if we regard the process of combustion as already explained, with, probably, also certain chemico-calorific processes on the one hand, in relation to the neurochemical with the vino-fermentive and certain other chemical processes, on the other; we shall, I think, find that they stand thus ætilogically divisible into two radical classes of phenomena, distinguished by having totally different exciting causes, and so far, therefore, coming under different *rationales.* Of these indications I shall have presently to avail myself, and of the results I shall only here remark that, as in combustion, the whole of the equilibric imponderable (of the combustible) is set free, and we have fire; while in the neurochemical and vino-fermentive processes, a part only is disengaged, and the result is caloric, and the same thing also occurs under the conditions predicated in other chemical processes. As to chemical relations and results generally, nothing need here be said. I know of no fact in the science opposed to what has been here advanced. I shall just allude to one brief example which now occurs to me. Why is a metal unoxidizable by heat, whose oxide is reducible by the same agent? Answer—Because the equilibric capacity of such metal must be less than that of oxygen gas, and thus the relation which the latter ordinarily bears in that respect to all oxidizable or combustible substances, is in such a case *reversed*, so that the metal, in fact, represents the place of the supporter, and the oxygen gas that of the combustible. I would not, however, pretend to say that their pyrific combination (unless on the score of physical disqualifications) might not still be possible, at least in theory, although the supply of the specific influence required (even by the oxygen gas) might be so in practice. But when the oxide is formed, not by pyrific, but by chemical combination, the conditions for such a result are merely that the oxygen be previously presented to the metal in the negative or active state, nearly in which it may be supposed to exist in the oxide; and when the reduction of the oxide is effected by electro-pyrific influence, this deficiency of the equilibric imponderable is then supplied. The bond of affinity is thus dissolved, equilibrium is restored, and the oxygen (positive or not) flies off, leaving the metal in its simple state. From what has been said we are now prepared to consider the pyro-lucific principle as the cause of light, from its being under certain conditions of evolution the material substance of that mood of electric development which, dependant on the power of pyrific attraction, determines the production of fire. Where this process cannot take place, there can be no electro-pyrific evolution, no pyrific light, and therefore we can have neither in the animal economy. To produce such a process a special profluent or positive electric influence applied to the combustive elements is indispensable, and where such an influence and such elements are found to co-exist, such a process must be the inevitable consequence. In the living organism, as I have shown, such an influence cannot—does not exist, and hence, alone, in the presence of all the other conditions, the absence of combustion. If, then, the elements in the neurochemical process are determined to a combination, attended with emission of caloric, but free from pyrific evolution, not only must the conclusion remain inevitable as to the *assumptive* nature of the exciting cause, but it would seem also to follow,—that as of the alimental combustible, the equilibric, (being an index of the free effluent,) must exceed the specific quantity, so should the *extent* of the excito-assumptive power not be less, at least, than the difference of those two quantities, otherwise it would, perhaps, be difficult (supposing the combination possible) to perceive how combustion should be evitable;* in fine, that this function—the supreme inceptive impulsion of the vital organism,—the very key of life, being the exciting cause of the neurochemical process, consists of neither more nor less than a power of electric assumption equal to the difference between the equilibric (of the combustible) and the actual calorific effluence, and that the latter is less than the pyro-specific, and combustion consequently precluded, precisely as the primary abstraction is greater than the difference of the pyro-specific and equilibric quantities.

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* That it is possible, and under certain circumstances inevitable—possibly from the causes here indicated—is shown from the fact of the occasional occurrence of spontaneous animal combustion.

In returning then again to the consideration of the first set of vital phenomena, I am fully borne out, at least, in submitting that the exciting cause thereof, as the supreme vital inceptive impulsion, *such* being the exciting cause of the neurochemical process, must consist of a vivo-electric assumption, varying with the equilibric quantity of the nutrimental combustible. In order, however, to render the proof of the position, as far as may be, complete, I have yet to refer to the experimental evidence as adducible from facts themselves, comprised in the economy of the non-igneous combination of the alimental elements with the oxygen gas of the air. But, first, supposing the blood to be duly charged with natural aliment, and this as the supreme inceptive function acting thereon,—how are we to explain the changes produced? Briefly thus:—The nervous power extracts from those atoms in contact with it the imponderable principle due to the neurochemic attraction, by which the equilibrium before existing between them and the oxygen gas of the atmosphere is of course destroyed, and they are rendered negative. This result takes place at the junction of the arteries and veins, *i. e.* in the true capillaries, exactly where the arterial fluid assumes the venous character. Of the elements thus prepared, a small proportion only combines with the free oxygen contained in the blood: while the chief bulk, combining with the unqualified elements of the surrounding mass, thereby completes the formation of the venous fluid, which remains in this state until its arrival at the surface of the lungs. Coming there in contact with the oxygen gas of the inspired air, the electric affinity is of course brought into play in favour of the latter and the qualified elements. They, consequently, at once combine; and, at the instant of their combination, the whole of the equilibric imponderable of the combustible base is given off in the form of a calorific effluence. Carbonic acid gas, watery vapour, &c. are formed, and carried off by expiration: while the eliminated caloric, retained, partly latent and partly free, by the now arterialized blood, is thus distributed throughout the system in the form of animal heat. I shall not stop to discuss explanatory minutiæ, nor to remark further on the rational grounds of the doctrine: I only regret that, not having the few necessary simple apparatus now at hand, I cannot myself put the matter to the test of experiment. Let us however suppose, for example, ℔j of fresh drawn venous blood to be inclosed in a flask capacious enough to contain also as much atmospheric air as would furnish the required oxygen gas for all the carbonic acid gas, &c. that could be formed by agitation. Now, I would ask, provided this were done at a similar temperature, should we not thus have precisely the same quantity of carbonic acid gas, &c. formed, as the same blood would have given off during its transit through the lungs? Ay, and the same caloric formed too. But at this point the process stops. And why? Is it that no more carbon and hydrogen exist in the now arterialized fluid? Of course the supposition cannot be admitted for a moment. In the organic system, the arterialized fluid is returned again to the capillaries, and being there subjected, by the supreme vital inceptive function to electric deprivation, is reconverted into true venous blood. If then the blood in our experiment having given off the whole of the carbon and hydrogen which had been prepared in the capillaries, has ceased to give more by further agitation in contact with air, solely for want (as is alleged) of further electrical deprivation; it of course follows that were it now to be submitted to the power of the negative pole of an electric or galvanic apparatus of proper strength, a parallel result should be the consequence, and the hydro-carbonic elements would be found to combine with the oxygen gas, &c. in contact with them, precisely as when qualified by the living system, and thus the most positive and incontestible proof be presented of the nature and identity of the qualifying power. But the indications of the experiment admit also of being fulfilled—perhaps not less satisfactorily—by other and still simpler means, requiring but a common stop-cock'd flask and an air pump. It is a well ascertained fact, that " rarified air admits the passage of electricity: so does the Torricellian vacuum: hence if an electrified body be placed under the receiver of an air pump, *it loses its electricity during exhaustion and bodies which when surrounded by air in its ordinary state give out no electricity, if submitted to a vacuum are found to emit it abundantly.*" Evidently, then, bodies subjected to such a process as this must be rendered negative in relation to those previously in equilibrio with them, and thus the blood in our experiment, if placed in the flask and the air extracted therefrom: its component parts thus acted on must derive a corresponding electrical qualification. Here, however, it is not to be forgotten that the degree or extent of electrical deprivation which might be thus effected, will, irrespective of the different equilibric affinities of bodies, depend also on several other circumstances; viz. the capacity of the vessel—the extent of surface regarding the rarified air—degree of exhaustion—surrounding medium and temperature—and, lastly, the kind of materials composing the vessel employed: by either or all of which influences the effect may be more or less modified. Thus, for example, if a *glass* vessel should be used: although the exhaustion might be complete; yet the electric imponderable would not escape, and consequently little or no effect would be produced: unless indeed a *metallic* stop-cock were attached, and this placed in contact with a conducting medium—as, for instance, when submersed in water, by which of course the imponderable principle would obtain a ready exit. I especially mention this, because apparently under precisely such circumstances as these were performed the two experiments I am now briefly about to notice, by Dr. Reid Clanny, as published in the *Lancet*, No. 565, and the results of which, I confess, appear to me to be so completely decisive of the great ætiological question at issue, as scarcely to leave room to regret my own want of experimental facilities.

In these experiments then I find, that from a quantity of venous blood received into a flask and treated with atmosphoric air, it was found possible to extract, by the formation (or eduction?) of carbonic acid gas, a certain or determinate proportion only of the free carbon contained therein, and on the loss of this it was observed immediately to assume the arterial hue. But, by abstracting the atmospheric air from the flask, turning the *stop-cock*, thereby inclosing the blood in a vacuum, and then immersing the *inverted* vessel in water, it was found that in twenty-four hours after the blood had recovered its venous character, and exhibited even a darker tint than at first. When, by a repetition of the former part of the process, another portion of carbon was extracted, and the blood immediately reassumed the arterial appearance. By again submitting it to the vacuum, and in like manner immersing it in water, a return of the venous colour it is stated was then observed; but less marked. and the quantity of carbon extracted was also less. It is sufficient for me to observe, that in the further repetitions of the process, the difference of the colour and the quantity of carbon obtained, appear to have diminished rapidly, so that on the eighth trial no more carbon could be extracted.

Now, having placed these results in juxtaposition with the foregoing, as an experimental test of the fundamental position to which they are intended to relate, I shall leave the reader to draw his own conclusions, merely remarking, that to me, the changes exhibited appear (as far as they go) to be so clearly analogous to those which the blood experiences in its circulation through the living system, the evidence accruing so complete and irrefutable, as to the common nature of the exciting cause of both series of changes, and the consequent ætiological fact so firmly established, of the electro-assumptive nature of the vital inceptive function, that I am totally unable, at present, to perceive on what grounds the proposition may be questioned. Having thus, from an examination of the neurochemical phenomena and their relations, arrived at what I have deemed to be the true nature of the exciting cause, as resolved by physiology—by the results themselves—by the collateral testimony of combustion, and by experiment, I have, finally, yet to refer to one other source of illustration, viz., analogy.

It is an indubitable fact, that the process of the vinous fermentation presents us, in like manner, with an example of the non-igneous combination of pyrific elements. And finding that the results are in this respect strictly analogous, the question to be considered is, must not that analogy extend to their exciting causes also? Now, whatever may be the nature of the intestine changes which take place in the economy of the vinous fermentation, the results assuredly are, on the one hand, the progressive formation of alcohol; and, on the other, as in the neurochemical process, the production of carbonic acid gas, accompanied by the evolution of caloric, unattended by fire. In applying to these phenomena the position affirmed, refering the non-igneous combination to an electro-assumptive agency, additional tests of its correctness may also, I presume, be found, from the points of difference distinguishing the present case, as well as more decisive evidence in regard to several of the secondary questions. Thus, the impossibility of ascertaining the amount of the effluent caloric, by *experiment*, in the neurochemical combination, does *not*, I think, exist in the present case; so that we might, therefore, by comparing the effluent caloric of the excreted carbon and oxygen with the pyrific complement of the same quantity of carbon burnt in air, demonstrate at once the important truth, that the former is in reality less than the latter. The amount of the difference, expressive of the abstract theorem, might also be compared with the pyrific complement of the alcohol formed; and the difference existing would be an index of the difference of the electric states of carbon and oxygen, as existing in sugar, and when isolated in air. Again, the fact (alleged) that the difference between the amount of the effluent caloric and the pyrific complement of the decomposed sugar, would represent the pyrific complement of the alcohol formed, might likewise be estimated by direct comparison. Or, lastly, the position, that the pyrific complement of the saccharine matter decomposed is equal to the sum of the effluent caloric and the pyrific complement of the alcohol formed, would admit, also, of similar investigation. But does not the abstract theorem stand also attested by the *rationale* of the *ensemble* of the known results? Does it not supply an explanation, and that the only one which would render, under the circumstances, in some degree accountable, the formation as well of the alcohol as of the carbonic acid gas? Let these questions be answered by a brief reference to the facts themselves. It would appear, from the atomic constitution of saccharine matter, that three equivalents of sugar contain the elements exactly of one equivalent of alcohol, and one equivalent of carbonic acid gas. The conformable explanation of the changes would therefore simply amount to this, that by an electro-assumptive power, which must be exercised by the segregated elements of each equivalent of alcohol, so much of the imponderable principle is extracted from the *third* carbonic atom, as suffices, on the one hand, to complete the alcoholic complement, and on the other, to qualify that carbonic atom for non-igneous combination; that, consequently, this negatived carbon combines immediately with the two remaining atoms of oxygen, forming an equivalent of carbonic acid gas, while the evolution of the remainder of the equilibric imponderable then ensuing, is disengaged in the form of sensible heat. Thus, by the assumption of the electric imponderable on the one hand, and the loss of it on the other, we have the production of alcohol and carbonic acid gas precisely under the conditions predicated, and have, accordingly, in this as well as in the difference in capacities of the bodies, but the fullest corroboration of what has been previously advanced. As to the circumstances (as from the presence of yest, &c.) which determine the segregation of the quiescent elements of the alcohol, and consequently the development of the electro-assumptive power by which the results are effected, nothing need here be said; it is sufficient to have arrived at the demonstration of that power in *both* cases. In the neurochemical process it constitutes, beyond all doubt, the *primitive fact*, beyond which it must be vain and absurd to speculate. It is not, therefore, necessary to extend the enquiry beyond its parallel here, especially as the attempt might, nay, certainly would, as far as my knowledge goes, prove quite as fruitless in the one case as the other. And here, then, as far as relates to the grounds and exposition of the great ætiological questions which I had ventured to propose submitting to the reader's notice on the present occasion, I might be content to bring this Essay to a close, convinced, that if farther commentary were necessary, it should either be such as I may not now adduce, or that the reader will himself better supply. I feel, however, tempted, by way of sequel, and for the further purpose of mutual illustration, to advert again, with brevity, to a few facts in physiology, which, as they seem to bear especially on the foregoing views, and are, I suppose, hardly explicable by any extant theory, may not be inappropriately considered in this place.

And first let us take the well known fact, that an increase of voluntary muscular exertion occasions invariably a corresponding acceleration of the motions of the heart and of the function of respiration, accompanied by an increased production of animal heat, &c. How is this to be accounted for? For my own part, I can only repeat that I know of no explanation hitherto offered, fitted to conciliate the assent of even the most superficial inquirer, and therefore it is that in consonance with the above views, I would now submit the following. All muscular action, of course, involves an expenditure of nervous power. The two systems of muscular motions are excited or determined by two distinct systems of causes. The exciting cause of voluntary motion resides in the nerves of volition. That of involuntary motion in special states of the blood. How then is it that the voluntary interfere with the involuntary organic actions, *i. e.* with those of the heart and respiration? That it *must* be in the first instance by a reaction of some part of the organic nervous apparatus on the blood itself, seems indeed sufficiently obvious. But may not that function be signalized and its *nature* demonstrated? I think so, and simply thus. The exercise of voluntary muscular motion must of course involve a corresponding expenditure of nervous influence. An increased expenditure of nervous influence must demand and occasion a corresponding increase in the supply thereof, and this increase can only be obtained through a corresponding acceleration in the functional action, by which alone it may be *assumed* and conveyed from that fount of all nervous power, the blood. And what is that function? If, indeed, the answer to this question stood not already sufficiently revealed. If there could be otherwise a doubt about the matter, certainly the demonstrable fact of the effects producible by an increase of demand, being precisely those of acceleration of the supreme inceptive function, would serve to fix at once, and beyond all doubt, their identity. The results then, by the increased activity of *that* function, as brought about by this or any other analogous cause, are, and must be, a corresponding acceleration of the conversion of the arterial into the venous blood, or in other words, the negativing or setting free of the carbon, &c. of that fluid. And this, by the consequent influence on the succession of exciting causes and to the end that the blood may be preserved in its normal state, necessitates (no matter how) a corresponding expedition of the motions of circulation and respiration, attended, of course, by augmented emission of animal heat, excrementition of carbon, &c., intromission of oxygen gas, &c.; and, lastly, and not least important, increased consumption of the pubular elements of the blood. How manifestly different the quantity of food consumed by the sedentary individual from that devoured by the man of active exercise! Hence then we may see how it is that the animal and organic motions are connected, and that when the former influence the latter, it is effected solely by, and the results are, those of an induced compensative reaction exercised on the blood by the supreme inceptive faculty of the vital organism, and furnishing, too, farther proof, were it wanting, of the existence and nature of that function. Between the development of the above *ensemble* of phenomena and the function of the vital inceptive nerve, a constant ratio must, in fact, and does exist; so that the comparative conditions of the former furnish us therefore with correct indices of the varied states of the latter, whether retarded or accelerated, and from whatever causes, direct or indirect, innate or accidental, transient or permanent. This seems, perhaps, so obvious, that to refer to further facts in evidence, unless illustrative of some practical bearing, might be deemed superfluous. I shall therefore merely remark, and in reference chiefly to the latter respect, that these views may help us to explain, as far as they go, why it is that the accelerated actions, &c. of the infantile economy differ so widely from the diminished actions and heat, retarded respiration, and slow pulse of old age. Why that in certain states of disease, as the hot and cold stages of intermittent fever, cholera Asiatica, &c,* or under certain mental influences,—as anger and fear—we find precisely similar differences presented by that same order of phenomena. And, finally, it might be added, why, not only the most potent vital invigorators, but also the most deadly instant poisons develope their characteristic effects *as such*, solely on these phenomena.

The next fact I would advert to here, is, an experiment performed, I believe, by Sir B. Brodie, and an account of which I have frequently met, although I cannot at the present moment refer to it. It went, however, (if I mistake not) to show, that after *decapitation* of an animal—the process of respiration being kept up artifically—the circulation of the blood may still persist. That, in fact, the same apparent changes in that fluid, accompanied by the extrication of carbonic acid gas, &c. are found to occur as *during the life* of the animal; yet, that notwithstanding this, no heat is generated, or indeed that the animal cools even more rapidly than it would have done spontaneously. How far these remarkable results may be found in unison with the foregoing views, remains to be seen. Unless, however, those views be based in error, little difficulty will arise in according them in the main with, and explaining by them, the stated phenomena, and, therefore, notwithstanding any difference of *version* not opposed by positively ascertained facts, in attesting once again to the truth of the theory. In reference to such an experiment, then, it is first to be remarked, that (if) the immediate result of decapitation would be a complete annihilation of the supreme inceptive faculty of the vital organism, and that consequently from that instant no more *venous blood* could be formed. Yet, however, that as the whole of the blood then existing in the veins must of course be already *venous*, *that* and that alone must therefore be already prepared to subserve those uses exclusively assigned to it in the living economy. What are those uses in relation to this experiment? Premising the heart's excitability, it must be evident according to the foregoing views, that as long as a flow of such venous blood exists for the supply of the right cordal cavities, so long will continue its propulsion through the lungs; the formation of carbonic acid gas, &c., and the consequent evolution of heat at the lungs, the conversion of the venous into arterial blood, and finally, its return to the veins by the action of the left cordal cavities. But this is the extent of what can take place, for the organic nervous apparatus being dormant, we can neither have the *conversion of the arterial fluid into venous*,* and the concomitant assumption of power at the one end, the union of carbon, &c. with the free oxygen of the blood, and the *consequent evolution of caloric*, immediately dependent on it. Nor the formation of any of the secretions, &c. (at least too trifling to notice) and the consequent disengagement of caloric at the other end. Hence, then, it would appear that of the three sources of animal heat in universal activity during life, one and only one can persist after death, *i. e.* that attending the combination of the carbon, &c. of the venous blood, with the oxygen of the inspired air at the surfuce of the lungs, and *as much only* of the heat so generated as the blood may be capable of *taking up and distributing*, may be imparted to the body in such an experiment as that now under consideration.

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* In assigning partial paralysis, or suspension of function, of the supreme organic nerve as the cause of Asiatic cholera, I consider that I am fully borne out both by its pathology and treatment.

Now it has been demonstrated by experience that the sensible temperature of the blood in traversing the lungs, even during life, is raised scarcely *one degree*. It is of no importance here whether this be the whole of the heat *then* formed, or, as is the more probable, a small proportion only, the rest becoming latent in the arterial fluid, although it would follow that the inceptive assumption must correspond in either case.† The fact is, however, certain, that in the dead animal there can be *no* elimination of heat from further changes in the blood, which can only be wrought by the agency of the living nerves (whether such elimination should be due in part or not to an imbibition of latent heat in the parenchyma of the lungs), and that, therefore, the capacity of the blood, the only remaining resource in such an experiment, to impart heat to the body, must be confined solely to the actual access of temperature imparted to it in its transit through the lungs. Hence, of the three sources of animal heat existing during life, thus much alone can remain after death. Consequently, the temperature of the living economy cannot be maintained during artificial respiration after death; and an animal thus experimented upon must cool, even while the blood continues to undergo the usual apparent changes in its passage through the lungs. But the supply of venous blood existing at death must soon become exhausted, and there being no power to reconvert the arterialized fluid into venous, the whole will have become arterialized. Those changes must cease, and the small generation of heat previously existing must then and for ever be at an end.

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* Such is the case, too, during *Syncope*, and there also the same phenomenon is consequently observed.

† I am aware that the tendency of experiments published, some especially, *since* the writing of this Essay, is to show that the whole of the carbonic acid is formed and contained in the venous blood *previous* to its arrival in the lungs, whence it is dislodged by the intromission of oxygen, &c. But, as this is a secondary point, and cannot affect the main deductions of this Essay, it may, for the present, be left in abeyance.

Such, as I should conceive, may be the true version of the results of such an experiment. In connexion with the heart's excitability, I shall now say a few words on general irritability. That this is an accidental property conferred on the mobile structure by some organic proceeding ancillary to the vital economy, and that, that which may be found in the dead muscle is merely the unexhausted surplus of such property remaining at death, will, I suppose, be readily admitted by all physiologists of the present day. But what the particular function may be on which its production depends, the nature of the endowment, its relations to other phenomena of the system, &c., are questions about which we seem to be as yet almost entirely uninformed. If, however, the views, for which I have had to contend, be based in truth, it may not be vain to expect that our knowledge may yet become more precise in relation to these interesting points. If it be granted (as I think I have shown it must), that all muscular action involves a corresponding consumption of the imponderable principle taken from the blood during its passage through the capillaries, unless the whole of that consumption then be supplied from the voluntary nerves (spinal chord) in one case, and from the blood in the other; it is clear that the organic property, termed irritability, must, if it have any thing to do in the business, consist of a positive expenditure of the nervous principle, and whether, indeed, as one of the two essential elements of motion, pervading universally the mobile tissues, this function be not ultimately the *sole positive* expenditor of the nervous principle in every action of the economy, might also be a question worth considering. That it is so in respect of the *organic* actions when the other element of motion is derived merely from the circulating fluid in its various states, appears to be self-evident. And what may be the function of the voluntary nerves if not an analogue of these stimuli, adapted to the irritability of the voluntary muscles, precisely as they are to the same property of the involuntary parts? However, the inference of more immediate import is, that the function of irritability consists of a positive expenditure of the nervous principle; an induction which when considered also in relation to the nature of the supreme inceptive faculty seems, indeed, the only one which would be conformable in theory. As to the site and organism of these two functions, I shall merely here recur to a conjecture which I have already advanced, viz. that they are twin functions, and may therefore in accordance with a general law of the economy belong ultimately to one and the same division of the nervous system, perhaps to the great sympathetic or ganglionic complexus of nerves; a supposition which, independently of other reasons, seems to me to derive some countenance from the peculiar structure, arrangement, and locality of that portion of the nervous system.

From muscular motion I turn, in conclusion, to secretion and nutrition. What is it that enables the various organs and tissues of elaboration to select respectively from the circulating mass of compound elements, and retain in preference to all others those only constituting their peculiar secretion, and not only this, but to qualify (electrically) those elements for the formation of the secreted matter? For, be it remarked, this cannot result from a single elective power, as in scarcely an instance, if any, does such matter previously exist in the blood. To this it may be answered, that beyond the fact of the existence of that principle which qualifies the muscles and mobile tissues of the system, to respond to their respective stimuli; and the intimate connexion and strict dependence shown to exist between the functions of those muscles (and tissues), and *that*, which in connexion with external agencies, originates the nervous power, as well as the impossibility of such functions, either being otherwise supplied or the latter power otherwise expended, all clearly demonstrating the identity of the operating principle with that abounding in the aliment:—We may here, perhaps, trace in the elective electro-qualifying properties of the nervine extremities presiding over secretion, &c., another and distinct proof of such identity, and therefore additional confirmation, so far, of the truth of the past conclusions. This compound elective electro-qualifying system of nerves may not inaptly be compared to an electro-galvanic apparatus, in respect of operative principle, agency, and results, and these nervine extremities may be accounted merely as an assemblage of decomposing wires of various degrees of power, possibly of one kind, having their ends immersed in the vital stream. The vital function of elective power cannot, indeed, be other than simply a relative special impulsion (be it active or passive, &c.), resident in the nervous tissue. Nor can the conclusion be avoided, that to it alone, as the basis of every organic process, must be ascribed the production of the phenomena connected with secretion, &c. And, as in the inorganic results of electro-galvanic agency, the existence of the same operative principle (identical with that of all power), must be admitted; so also may we find, as in those results, that at least an equally adequate train of causes may be assigned for the production of organic effects. First, have we not the *special* or electric power of each set of nerves to take from the blood such of its elements as fall within the projected sphere of affinity? Hence the peculiar matter of the secretional organ. Must not the aggregated molecules derive corresponding electric qualifications, dependent on their radical distinctions, the state in which, and the power (with the consequent eliminated influences) by which attracted? Hence, then, the peculiar electric qualification, which by giving rise to consequent changes, ending in the formation, determines the character of the elaborated matter. During these combinations may it not be inferred, that an imponderable emission, *from the elements*, takes place in the shape of caloric.

Furthermore, and finally, as the secretional formations, the blood and the voluntary nerves stimulating the mobile tissues, determine all the actions of the economy, involving as those actions do a consumption of the imponderable principle appropriated from the blood. Does it not follow, as a legitimate conclusion, that those actions (exclusive of course of what may be expended in qualifying the elaborated elements, and therefore) especially the involuntary, but above all, the *voluntary muscular motions* should be attended by a corresponding emancipation *from the organs themselves*, in one mood or other (distinct from the calorific) of that imponderable principle? To me it would appear so. But can there be any evidence adduced in support of the existence of such a phenomenon, in connexion either with the voluntary or involuntary motions? perhaps not. Still, however, the fact might be incapable of physical demonstration, or the hiatus may yet remain to be supplied by future observation. For my own part, I cannot indeed avoid observing that there may be found facts and materials, even within the limits of our present knowledge, seemingly not undeserving the closest scrutiny, in connexion with this deeply interesting question. Vide "Reports of the Experiments on Animal Magnetism, made by a Medical Committee of the Royal Academy of Sciences, Paris, and read at the Meetings of June 21st and 28th, 1831."

The reader may smile; but, of course, after what has been advanced, it cannot be supposed that *I* should be found either to discredit or question the statement of facts published by the commissioners on that occasion, who were indeed all men of the highest qualifications and character, the least liable to be practised upon, and of course wholly incapable of lending themselves to any species of collusion. Neither can I refer the results to disordered imagination. Such, then, being the case, I have only simply to ask, what do those facts amount to in reference to the point just mooted? Do they or do they not justify the foregoing inference? Does a transmission of a physical influence, I mean, of an imponderable operative principle take place during muscular motion from one human body to another, or to the surrounding media? That is the question; and I pause for a reply, not yet, perhaps, determined by actual experience. Whether the investigators in the above-named experiments might have thrown additional light on this point, or conferred greater certainty on the general results, by a comparative reference to *gravitation*, I know not. If, however, the phenomenon of gravitation be dependent on the causes and conditions, I have ventured to intimate in the early part of this paper, and if it follows, that by disturbing those conditions, as may be done by means of an electric or galvanic apparatus, gravitation *must* be affected accordingly. Why, then, I can only say, that at least it would seem neither impossible nor improbable, that by the agency of the same principle, as thrown off from the vital tissue, a similar effect might be produced. Let me here add, that Sir David Brewster (I think) has already somewhere recorded a very extraordinary fact, bearing especially, as it appears to me, on this very point. It is an experiment in which, if I mistake not, four men are enabled to sustain a fifth upon their fingers' ends, and from which I should say no other inference can be drawn than that there *must* have been a suspension, and that to no small extent, of the normal gravitation of the superincumbent body. There is one other phenomenon, which I would just notice in conclusion, as one which it appears by no means impossible might be found to be connected with the same cause. Hunter finding himself unable to discover the existence of any physical cause for the blood's non-coagulability, therefore imagined that fluid's homogeneity might be due to its *vitality* and its coagulation, to the absence of that condition, or the death of the blood. But it may be asked what grounds are there for such an hypothesis? What does it amount to? Does it imply any facts which would in any degree explain the phenomenon? Certainly not. In fact, the conjecture amounts to nothing, and we are as yet entirely in ignorance as to the cause of the phenomenon. Any suggestion which might lead to its explanation merits, of course, the physiologist's attention. The more so, as this seems especially to be one of those problems with which physiology abounds, pregnant, probably, with more than an isolated fact, of the importance of the solution of which, a correct judgment is necessarily only to be arrived at, *a posteriori*, and therefore is the discovery to be looked upon as the more desirable.

THE END.

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